

more powerfully on particles of ice than on drops of water. I venture to offer the following suggestions.

Let us consider what happens when an ice cloud is forming. Dust particles, no doubt, act as nuclei to ice crystals as well as to water drops; so that a number of crystals will start into existence about the same time. Soon there will be no more dust particles of sufficient size to form nuclei. The rate of deposition on a crystal will be proportional to its surface, so all the crystals will grow in diameter at the same rate. The ratio of the largest diameter to the smallest will become less. In fact the crystals will become more uniform in size. No doubt, too, the supply of aqueous vapour near a large crystal will be consumed more rapidly than it can be refunded by diffusion. So the larger crystals will grow somewhat more slowly. These are causes tending towards uniformity. They account for the observed fact that they are newly formed clouds, which show the brightest colours, though when they first come into view they are white. It is easy to give reasons why some clouds should lose their colour so soon. Varying conditions may affect the growth of different layers of particles in a different manner, or a slight increase in the rate of deposition may call new dust particles into action.

In the case of water clouds there are two special causes brought into action against uniformity. Sir William Thomson has shown that the maximum vapour-tension at the surface of water is largely increased when the surface is highly convex. So the large drops will grow more rapidly than the small ones, and the range of size will be more and more extended. Secondly, whenever two drops of water come into collision, they will combine into one larger one.

In conclusion I may remark that St. Moritz is 6000 feet above sea-level, and the iridescent clouds were generally above the surrounding mountains, *i.e.* at least 11,500 feet above sea-level. On some days the sky was overspread at a great height, with a thin haze gathered here and there into denser streaks (cirrostratus?). The haze sometimes formed coronæ close around the sun. I have not made out more than two spectra. From rough measures of the diameters of the rings, I find for the diameters of these filaments values varying from .04 mm. to .07 mm.

JAMES C. MCCONNEL

St. Moritz, Switzerland, March 14

Aino Hairiness and the Urvolk of Japan

IN Mr. B. H. Chamberlain's remarkable and instructive monograph on the Ainos, contained in the first number of the *Memoirs of the Literature College of the Imperial University of Japan*, just published, will be found an explanation of the different conclusions that have been arrived at by different observers as to the *hairiness* of that singular people, equally isolated, so far as our present knowledge extends, by language and by physical characteristics from all surrounding races. When I spent some days among these so-called savages in 1865 or 1866, I had the opportunity of examining some four or five score of them, chiefly men, and in every individual I found the phenomenon of hairiness more or less marked. The sternal, inter-scapular, and gluteal regions were, in particular, thus provided with a natural covering, the very regions where such a protection from the drip of rain would be most serviceable. I remember well that in some individuals the gluteal fur was so abundantly developed that thick tufts of hair, several inches long, could be grasped in the hand. But recent travellers have been struck by the number of natives they met with deficient in hairiness—whether they were proportionately lacking in face-hair is not stated—and it has been doubted whether hairiness is really an Aino characteristic. Dr. Baelz's investigations have, however, amply vindicated the claim of the Ainos to be the hairiest people in the world, and now Mr. Chamberlain shows that the smooth-bodied natives are in fact half-breeds, the progeny of native mothers by Japanese fathers. Unions of this kind have probably increased in frequency during the last two decades. Between the two races, however, some incompatibility seems to exist, for their progeny exhibit a diminished fertility. "The second generation," says Mr. Chamberlain, on the authority of the Rev. Mr. Batchelor, who has lived for years among the Ainos, and contributes an exhaustive grammar of their language to the volume of *Memoirs* before me, "is almost barren; and such children as are born—whether it be from two half-breed parents, or from one half-breed parent and a member of either pure race—are generally weakly. In the third or fourth generation the family dies out."

The injury to the reproductive system caused by this "miscegenation"—a phenomenon not unparalleled in the history of man, and proving the existence in man, as in other organisms, of a tendency to specific variation—has an important bearing upon the much-debated question of the proportion of Aino blood that runs in the veins of the Japanese of to-day. Mr. Chamberlain, chiefly from philological considerations based upon an examination of place-names, arrives at the conclusion that the *Urvolk* of the Japanese group, from the extreme south to the furthest north, was an Aino race, and we know from history that up to the time of Yoritomo (twelfth century A.D.), and probably later, the northern half of the main island was still, to some extent, peopled by Ainos. Yet even the northern Japanese are smooth-bodied, although it is extremely unlikely that "miscegenation" did not obtain between their Japanese ancestors and the aborigines. In the light of Mr. Batchelor's observations the explanation of this apparent anomaly is easy. The half-breeds died out, and the prepotency of the Japanese in numbers and civilisation gradually expelled the Aino element from the population, which has thus become an almost purely Japanese one.

It must not, nevertheless, be forgotten that at least two distinct races may still be traced in the existing population of the Japanese group. One is slim, high-headed, and often aquiline-nosed; the other stouter and broader, more brachycephalic, and flat-nosed. Excellent types of both, especially of the first, will be found in Siebold's "*Nippon Archiepf.*" The former constituted the military class of Old Japan, the latter the peasantry; and of the latter some degree of hairiness of the limbs is a not uncommon characteristic. The drawings of Hokusai sufficiently prove this assertion, which the experience of every resident in Japan will confirm. It may therefore be safely assumed that the elimination of the Aino element has not been complete. For my own part I believe that the earliest inhabitants of Japan were tribes of Malayo-Polynesian blood coming from the south, and of Aino blood coming from the north. Altaic immigrants followed, and, partly perhaps through some degree of reproductive prepotency, gave a characteristic and predominant stamp to the population without total elimination of its aboriginal elements.

University of London, March 21

F. V. DICKINS

Units of Weight, Mass, and Force

HAVING read with much interest Prof. Greenhill's letter in *NATURE* of March 24, p. 486, I am inclined to think that much of the perplexity felt by some who begin the study of dynamics arises from the want of names for the units of the various magnitudes with which the science deals. We have names for units of time, space, mass, force, work; but no names for units of velocity, acceleration, impulse, momentum, &c. I venture to suggest the following:—Let the unit velocity be that with which a point describes uniformly one foot per second. Let this unit be called a *vel*. Let the unit acceleration be that whereby the velocity is uniformly changed by one *vel* per second. Let this unit be called a *cel*. Then everything becomes clear. *E.g.* the meaning of the equation $W = mg$ is seen to be this: The weight of a mass of m pounds at a place where the acceleration arising from the mutual stress between it and the mass of the earth is g cels being W poundals, the numbers Wmg are connected by the equation $W = mg$. Then, for the sake of the beginner, let the names of the units be given thus: $W = mg$ poundals, $m = \frac{W}{g}$ pounds, $g = \frac{W}{m}$ cels. It is too common to see acceleration expressed in feet per second, instead of in *vels* per second. If the weight of a mass of m pounds be defined to be the mutual stress between it and the mass e pounds of the earth, it is evident that the weight of e attracted by m is the same as the weight of m attracted by e ; and, in the absence of either, the other would have no weight.

Bardsea, March 29

EDWARD GEOGHEGAN

The Earthquake in the Riviera

THOUGH there can be no question as to the amount of damage done by the late earthquake—I am writing in a shed, the hotel being destroyed—I think that the violence of the shock has perhaps been very greatly exaggerated. I have only been able to make a flying visit to this place and to Diano Marina, but I cannot help being struck by the fact that the peculiar architecture is the main cause of the large amount of destruction

Indeed, judging from the small amount of damage done to buildings in which flat or square stones and flat floors have been used, I should doubt whether the shock was much more severe than that which not long ago damaged the brick buildings in East Anglia.

At both Oneglia and Diano Marina the building material is usually rounded stones from the beach, or rubble with stones of all shapes and sizes. The stucco is apparently expected to make good any deficiencies. Besides this the floors are nearly always brick arches abutting against the vertical walls, without any reference to other lateral support. Most of these houses are three or four stories high. Of course any vibration affecting buildings of this construction will split the walls in all directions, for besides the lateral thrust of the arches, the walls are full of wedge-shaped stones ready to slip into any fissures which may form.

The complication caused by these arched floors makes it very difficult to trace the direction or angle of emergence of the shock.

CLEMENT REID

Oneglia, March 13

Scorpion Virus

ALLOW me to state that the results of my experiments on Cape scorpions are in full accord with Prof. Bourne's conclusion that the poison of the scorpion has no fatal effect on the same individual or another individual of the same or even of another species. Speaking before the South African Philosophical Society in February 1883, I said:—"Members of the Society will see on the table a scorpion of the larger (Cape) species. That scorpion I caught at 11 o'clock this morning. I at once pierced him in three places with his own sting, on which in each case there was a drop of poison. In the last inoculation I held the sting in the wound, and squeezing the 'bulb of the sting' with the pincers forcibly injected poison. The creature is alive and active" (Proceedings for 1883). These and subsequent experiments, however, led me to believe that the poison has some effect, causing sluggishness and torpor for a while. I quite agree with Prof. Bourne that it is physically possible for a scorpion to sting itself in a vulnerable place; and though I never was able to observe the infliction of a wound on itself by any scorpion, I can well believe that this is possible, but, I am convinced, wholly accidental.

I found also that the poison of the ring-hals snake (*Naja hamachates*) was not fatal when inoculated in the same individual or another individual of the same species.

University College, Bristol

C. LLOYD MORGAN

The Supposed Myzostoma-cysts in *Antedon rosacea*

SOME eighteen months ago I called attention in these columns (vol. xxxii. p. 391, and vol. xxxiii. p. 8) to certain malformations which I had discovered on the pinnules of *Antedon rosacea* from various British localities. They often take the form of small cysts which are very like those produced by encysting Myzostomida on the arms and pinnules of various Crinoids from the Pacific; and as no other cyst-builders but *Myzostoma* were then known to infest the Crinoids, the inference seemed a natural one that the cysts on the pinnules of *Antedon rosacea* had been produced by a small member of this genus. I subsequently found several more cysts on some examples of *Antedon rosacea* which were dredged at Gibraltar by the Italian corvette *Vettor Pisani*, and the whole collection was sent to my friend Prof. L. von Graff for examination.

To our great surprise, however, he has not found a single *Myzostoma* in any one of the fourteen malformations of the pinnules, whether cysts or otherwise, which he has opened; and "the new British *Myzostoma*" must therefore be disestablished.

But what, then, has been the cause of these malformations? Prof. von Graff has found them to be always associated with the presence of a minute globular body, which has the appearance of an egg that has undergone superficial cleavage, but yet exhibits no trace of nuclei when stained. It is impossible to decide at present what this structure may be. Prof. von Graff has described it more fully in a "Supplementary Report on the Myzostomida of the Challenger Expedition" which he has just completed. But its nature seems to be as problematical as that of the sacculi; and fresh material, not spirit specimens, must be examined

before we can expect to learn much more about it. In any case, however, it would seem that we have to deal with a hitherto unknown parasite of the Crinoids, which is capable of producing modifications in the calcareous tissues of the arms and pinnules, of essentially the same character as those caused by *Myzostoma*, though of smaller size.

I would commend the question to the attention of those naturalists who may meet with *Antedon rosacea* in the dredgings of the ensuing season; and in order that they may know how to catch their hare, I shall be most happy to forward specimens of the cysts to anyone who desires to become acquainted with their external appearance. I may add that the largest cysts I have seen are on Comatulæ from the Cumbre, Milford Haven, and Gibraltar; while I have no knowledge of their occurrence either at Naples or anywhere else in the Mediterranean.

Eton College

P. HERBERT CARPENTER

On some Observations on Palæobotany in Goebel's "Outlines of Classification and Special Morphology of Plants"

THE few modern authors of botanical text-books who have ventured to summarise recent palæobotanical researches have achieved but moderate success. These authors have too little knowledge of the rapid progress of the study of fossil plants during the last few years to make success possible; hence, their summaries, if not absolutely inaccurate, are usually misleading. So long as these errors are confined to works published in Continental languages, British palæobotanists need not take the trouble to correct them. But the case is altered when English translations of these books appear amongst us. Palæobotany has nowhere made greater progress during the last few years than with ourselves. Many errors have been corrected, and new truths, results of careful and prolonged investigations, have taken their place. With the more important of these new discoveries many of our younger students of geology are now familiar. It is desirable that what they have been taught should not be contradicted by the utterances of authors ignorant of the subjects upon which they venture to express an opinion.

Some little time ago Dr. Goebel, of Rostock, published a volume which was virtually a new edition of Book II. of Sachs's "Lehrbuch der Botanik," and an English translation of this volume, made by Mr. H. E. F. Garnsey, and revised by Prof. I. B. Balfour, has just appeared. Dr. Goebel's volume contains some references to the Palæozoic flora which are seriously behind the times. To allow these statements to reach our students uncorrected will do harm, because they must suggest to those students that certain questions are still open and debatable which cannot now be regarded as such.

Had I not unfortunately misunderstood a wish expressed by my friend Prof. Balfour, some explanatory footnotes would have been introduced into the above volume, which would have rendered this communication unnecessary.

On p. 193 of the translation we find the following statement:—"Other groups are the Sphenophylleæ, Lepidodendrea, and Sigillariæ, the first of which are only heterosporous Lycopodiaceæ." We have no reasons for concluding that Sphenophyllum is Lycopodiaceous, still less that it is heterosporous. This latter statement rests upon M. Renault's interpretation of a minute multicellular fragment which he observed in a sporangium, and which he believed to be a macrospore; it could not be this, since the exosporium of a macrospore is a unicellular organism. On the other hand, the Lepidodendrea were both homosporous and heterosporous. As to the Sigillariæ, even M. Renault now admits that all the vertically-fluted forms are Lycopodiaceous. The assertion that the Lepidodendrea were all heterosporous is repeated on p. 196. On p. 272 we have a brief paragraph of eight lines remarkable for the number of the inaccurate statements which it contains. I have indicated these inaccuracies by reproducing them in italics.

"The Calamites are Equisetaceæ which appear in the older geological formations, beginning in the Carboniferous Limestone (1), culminating in the Coal-measures, and disappearing in the Permian formation. The spikes of sporangia are either not known, or so badly preserved (*Calamostachys*) (2), that their structure cannot be determined; it remains doubtful, therefore, whether they were homosporous or heterosporous forms (3). The stems had neither leaves nor leaf-sheaths, or else these were transitory formations and soon fell off (4). In other respects the structure of the stems resembles that of the Equisetaceæ (5).